When Do Law Enforcement Policies Generate Economic Spillovers?*

Evidence from 287(g) Agreements

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Abstract

Who bears the economic costs of local immigration enforcement? We exploit the staggered adoption of voluntary 287(g) agreements between county law enforcement agencies and Immigration and Customs Enforcement to decompose enforcement effects into own-county and neighbor-county components. Using a county-level annual panel covering approximately 3,100 U.S. counties from 2010 to 2023, we document a striking spatial asymmetry: enforcement has no detectable effect on wages or employment within the enforcing county, but depresses wages by 2–4 percent in neighboring counties, particularly in immigrant-intensive sectors such as accommodation and food services. Rents rise in both enforcing and neighboring jurisdictions, consistent with displaced populations increasing regional housing demand. English Language Learner enrollment shifts to non-sanctuary neighbors, confirming a population reallocation channel, although we find no corresponding fiscal burden in receiving jurisdictions. These results indicate that the labor market costs of 287(g) enforcement are spatially exported to neighboring jurisdictions rather than borne locally — a spatial externality that standard within-jurisdiction analyses miss entirely. Our findings contribute to ongoing policy debates by demonstrating that the incidence of enforcement falls not on the jurisdictions that choose to enforce, but on their neighbors.

Keywords: immigration enforcement, 287(g), spatial externalities, labor markets, wages, housing, sanctuary cities

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1 Introduction

Local governments routinely adopt policies — zoning, taxation, policing, welfare eligibility, sanctuary designation — whose benefits and costs are presumed to fall on their own residents. A substantial public-economics literature has shown this assumption to be unreliable: tax competition (Wilson, 1999; Brueckner, 2003), welfare competition (Brueckner, 2000; Berry, Fording, and Hanson, 2003), minimum-wage setting (Dube et al., 2010), and environmental regulation (Kahn and Mansur, 2013) produce measurable spillovers into neighboring jurisdictions. Immigration enforcement is the latest policy domain where the question of spatial incidence has become first-order. In 2023, approximately 160 U.S. counties maintained active 287(g) agreements with Immigration and Customs Enforcement (ICE), authorizing local law enforcement to perform federal immigration functions during routine encounters. Unlike the mandatory Secure Communities program studied by East et al. (2023), 287(g) is voluntary and selectively adopted — a policy design that produces geographic clusters of enforcing and non-enforcing counties and, in turn, the variation needed to ask who bears the economic costs of a county's decision to cooperate with federal immigration authorities.

We address this question by decomposing the labor market and housing effects of 287(g) enforcement into own-county and neighbor-county components. Our identification strategy exploits the staggered adoption of 287(g) agreements across approximately 120 counties between 2006 and 2024. For each county-year, we construct a spatial exposure measure — the share of contiguous neighbors with active 287(g) agreements — and estimate its effect on wages, employment, rents, and school enrollment. County fixed effects and state-by-year fixed effects absorb time-invariant county heterogeneity and state-level trends that might confound the relationship between enforcement exposure and economic outcomes.

Whether local enforcement generates cross-jurisdictional economic spillovers is theoretically contested. We argue that enforcement should generate measurable spillovers through three channels: Tiebout-style sorting under enforcement risk, in which displaced residents relocate to the nearest non-enforcing jurisdiction while preserving labor-market access; a neighbor-county labor supply shock, as the inflow of displaced workers raises effective supply in adjacent counties; and employer relocation in immigrant-intensive sectors. Several considerations work in the opposite direction. 287(g) operates through workplace and jail enforcement whose legal reach stops at the county line; commuting costs and family ties anchor residence; informal labor markets can absorb shocks locally; neighbor counties may lack complementary industries; and general-equilibrium price adjustments through housing and wages may offset sorting pressures (Monras, 2020; Borjas and Cassidy, 2019). The spillover prediction is therefore ex ante ambiguous, and the magnitude of any such spillover is an empirical question.

We report three main findings. First, we document a striking spatial asymmetry in wage effects. Own 287(g) enforcement has no detectable effect on wages across five industry sectors (total private, construction, agriculture, accommodation and food services, administrative and waste services), although neighbor enforcement depresses total private-sector wages by approximately 3.1 percent. When both variables are included simultaneously, the own-county effect remains indistinguishable from zero and the neighbor-county effect remains a statistically significant 3.1 percent decline. The wage depression is concentrated in immigrant-intensive sectors, consistent with a labor supply channel. Second, rents rise in counties exposed to neighbor enforcement, and the pattern is not limited to sanctuary jurisdictions. Third, English Language Learner (ELL) enrollment increases by approximately 22 percent in non-sanctuary counties neighboring enforcers, tracing the population reallocation that underlies the wage and rent patterns, although we find no corresponding increase in poverty rates, SNAP utilization, or incarceration in receiving counties.

Our paper makes three contributions. First, we are the first to estimate cross-jurisdictional labor market effects of immigration enforcement. The existing literature (East et al., 2023; East and Velasquez, 2024; Ali et al., 2024; Gutierrez-Li et al., 2025) estimates effects within enforcing jurisdictions, and the dominant policy setting — Secure Communities — was universal and mandatory, leaving no non-enforcing neighbors to absorb displaced workers. Our spatial decomposition recovers a spillover that universal-program designs mechanically cannot. Second, we import a public-economics spillover framework (Brueckner, 2003; Kahn and Mansur, 2013) into the immigration-enforcement literature, which has centered own-jurisdiction difference-in-differences and treated spatial incidence as a robustness check rather than the object of interest. Third, we contribute to the active policy debate over local cooperation with federal immigration enforcement: the 900-percent expansion of 287(g) through 2025–2026 has made the neighbor-incidence question first-order for state and local officials who neither signed the agreements nor can opt out of proximity to counties that did.

The remainder of the paper proceeds as follows. Section 2 reviews the existing literature on policy spillovers, immigration enforcement, and local labor markets. Section 3 describes the institutional background of 287(g) agreements. Section 4 develops the theoretical framework and generates predictions. Section 5 describes the data sources and variable construction. Section 6 presents the research design. Section 7 reports the main results. Section 8 concludes.

2 Literature Review

Our paper sits at the intersection of four literatures: the public economics of policy spillovers across jurisdictions, the labor market effects of immigration enforcement, the economics of immigration and spatial equilibrium, and the fiscal politics of sanctuary jurisdictions. We review each in turn, identifying the gap that our spatial decomposition addresses.

2.1 Policy Spillovers Across Jurisdictions

A large body of work in public economics examines how local policies spill over to neighboring jurisdictions. The canonical cases involve fiscal competition. Wilson (1999) formalizes the theory of tax competition, and Brueckner (2003) surveys the broader framework of strategic interaction among governments: jurisdictions set tax rates, expenditure levels, and regulatory standards as reaction functions of their neighbors' choices, generating spatial interdependence even when no explicit coordination exists. Brueckner (2000) and Berry, Fording, and Hanson (2003) extend this logic to welfare competition, showing that states set AFDC and TANF benefits responsive to neighboring states' generosity in order to manage the in-migration of recipient households. The empirical analog for local labor market policy is Dube et al. (2010), whose contiguous-county border design pioneered the estimation of minimum-wage effects across state boundaries and whose methodological approach is the direct ancestor of our queen-contiguity design. Kahn and Mansur (2013) apply the same framework to environmental regulation, documenting that polluting industries sort across county lines in response to differential regulatory stringency — a domestic pollution-haven effect that is directly analogous to the employer-sorting channel we consider.

A parallel literature examines the geographic displacement of enforcement activities. González-Navarro (2013) shows that Lojack adoption in Mexican states displaced approximately 18 percent of thefts into non-adopting neighbors, establishing that the benefits of targeted enforcement can leak across jurisdictional borders. Blattman et al. (2021) find that hot-spots policing in Bogotá produced measurable property-crime displacement to adjacent street segments. In the immigration domain specifically, Kang and Song (2022) develop the spatial decomposition of Secure Communities effects that is the closest methodological precedent for our own design, although they study crime rather than labor markets. Hansen et al. (2020) document spillovers from marijuana legalization in Colorado and Washington into the border counties of non-legalizing neighbors. Martínez-Schuldt and Martínez (2019) examine sanctuary designation and find no robust crime spillovers, suggesting that the spatial incidence of immigration-related policies operates through channels other than crime.

Two features of this literature are important for our argument. First, the methodological infrastructure for estimating cross-jurisdictional spillovers is well-established in public economics — contiguity-based spatial weights, own-versus-neighbor decompositions, and border-discontinuity designs are standard tools. Second, immigration enforcement has so far sat outside this framework. The enforcement literature has centered own-jurisdiction causal effects (East et al., 2023; Miles and Cox, 2014; Alsan and Yang, 2024), and the prior examinations of spillovers have focused on crime (Kang and Song, 2022) rather than labor and housing markets. Our contribution is to import the public-economics spillover framework into the immigration-enforcement setting and to document that the wage incidence of 287(g) operates at the neighbor-county margin, consistent with the broader pattern documented for taxes, welfare, wages, and environmental regulation.

2.2 Immigration Enforcement and Labor Markets

A rapidly growing literature examines how interior immigration enforcement affects local labor markets. The foundational contribution is East et al. (2023), who study the rollout of Secure Communities (SC) — a mandatory federal program that checked fingerprints of all individuals booked into local jails against immigration databases. Using American Community Survey microdata at the commuting zone level, East et al. find that SC activation reduced likely-unauthorized immigrant employment by 5.8 percent and wages by 1.3 percent. Crucially, they also document negative spillovers to U.S.-born workers: native employment fell by 0.55 percent and wages by 0.6 percent. They attribute these native effects to two demand-side channels: (1) a labor cost channel, in which the removal of cheap immigrant labor raises production costs and reduces job creation, and (2) a consumption channel, in which departing immigrants reduce local demand for non-tradable goods and services.

Subsequent work has extended East et al.'s framework along several dimensions. East and Velasquez (2024) show that SC reduced the labor supply of college-educated U.S.-born mothers by increasing the cost of outsourcing household services (childcare, cleaning) that had previously been performed by immigrant workers. Ali et al. (2024) document parallel effects in the child care market: SC reduced child care employment and participation, with downstream effects on maternal labor force participation. Rubalcaba et al. (2024) find that enforcement increases labor supply among U.S.-born Hispanic teenagers in mixed-status families, as adolescents compensate for parents who withdraw from the labor market due to deportation risk. Gutierrez-Li et al. (2025) demonstrate that enforcement arrests reduce farmworker labor force participation by up to 3.4 percentage points, with disproportionate effects in agricultural regions.

On the employer side, Orrenius et al. (2024) distinguish between worker-targeted and employer-targeted enforcement actions, finding that worksite enforcement raises employment (as firms substitute away from unauthorized workers) but reduces average wages, with employer-targeted actions generating larger effects than worker-targeted raids. Amuedo-Dorantes and Antman (2022) show that even the *awareness* of ICE raids affects worker engagement, suggesting that enforcement operates partly through fear and uncertainty rather than through actual removals.

Two features of this literature are noteworthy for our purposes. First, every paper in this stream estimates the effect of enforcement *within* the jurisdiction where enforcement occurs. Even when the outcome of interest involves spillovers across worker types (immigrant to native), the geographic unit of analysis is the enforcing jurisdiction itself. No paper in this literature estimates the effect of enforcement on *neighboring* jurisdictions' labor markets. Second, the dominant policy variation — Secure Communities — was mandatory and eventually universal: by 2013, every jurisdiction in the United States had been activated. The universal rollout means there was, in equilibrium, no "elsewhere" for displaced workers to go; the spatial externalities we study could not arise under SC because there were no non-enforcing neighbors. Our paper exploits the selective, voluntary nature of 287(g) agreements, which creates precisely the geographic variation needed to estimate spatial externalities.

2.3 Crime, Enforcement, and Geographic Spillovers

The closest methodological precedent for our spatial decomposition comes from the crime literature. Kang and Song (2022) study whether Secure Communities displaced crime into neighboring jurisdictions — the "balloon effect." Using a spatial model that simultaneously estimates own-jurisdiction and neighbor-jurisdiction effects, Kang and Song find that SC reduced crime locally only when neighboring jurisdictions were also activated, and that activation in some counties displaced crime into non-activated neighbors. Their design is the template for our own-versus-neighbor decomposition, although they study crime rather than labor markets.

The broader crime-enforcement literature has produced mixed results. Miles and Cox (2014) find no significant effect of 287(g) on crime rates, while Hines (2019) documents that Secure Communities reduced the FBI crime index, with effects driven by Hispanic communities. Chalfin (2015) reviews the evidence on immigration and crime more broadly, finding little support for the claim that immigration increases crime. In our own analysis of quarterly crime data, we find that the apparent relationship between enforcement and crime collapses to zero once state-by-quarter fixed effects are included, consistent with the null findings in this literature.

2.4 Immigration, Spatial Equilibrium, and Housing

The canonical spatial equilibrium framework in urban economics predicts that local shocks — including immigration — are capitalized into housing prices and wages (Rosen, 1979; Roback, 1982). Saiz (2007) provides the seminal evidence that immigration raises rents in receiving cities, with an elasticity of approximately 1 percent per 1 percentage-point increase in the population. Saiz (2003) shows that the Miami housing market absorbed the Mariel boatlift through rent increases rather than native out-migration, challenging the displacement narrative.

Our findings on enforcement and rents extend this logic: if enforcement pushes populations into neighboring jurisdictions, we should observe rent increases in those jurisdictions, as the effective demand for housing rises. The spatial equilibrium framework also predicts that wages should adjust to compensate for housing cost changes, although the speed and completeness of this adjustment depend on labor market frictions and the tradability of local production (Moretti, 2011). Our finding that wages fall in neighboring jurisdictions while rents rise is consistent with an adjustment process that is incomplete or that operates through different channels for different factors.

2.5 Immigration, Complementarity, and Substitution

The theoretical predictions for wage effects of enforcement-induced labor reallocation depend on the relationship between immigrant and native workers in production. Borjas (2003) emphasizes substitutability, predicting that immigrant inflows depress native wages in competing skill cells. Card (2001) and Peri and Sparber (2009) emphasize complementarity: immigrants and natives specialize in different tasks (immigrants in manual tasks, natives in communication-intensive tasks), so immigration can raise native wages through gains from specialization. Cortes (2008) shows that low-skilled immigration reduces prices of immigrant-intensive services (housekeeping, gardening, child care), which may benefit native consumers even if it depresses wages in directly competing occupations.

The enforcement context provides a distinctive test of these competing predictions. If enforcement displaces low-skilled workers into neighboring labor markets, the substitution hypothesis predicts wage depression in immigrant-intensive sectors; the complementarity hypothesis predicts null or even positive wage effects as the arriving workforce complements rather than competes with native labor. Our finding of wage depression in total private-sector wages — but concentrated in sectors such as food services — is more consistent with the substitution channel operating in specific segments of the labor market, rather than a general equilibrium complementarity effect.

2.6 Fiscal Federalism and Sanctuary Cities

The political debate over sanctuary cities frequently invokes fiscal burden arguments: that jurisdictions which refuse to cooperate with federal immigration enforcement must absorb the service costs of displaced immigrant populations (Oates, 1999). The Tiebout framework

(Tiebout, 1956) provides the theoretical foundation for this concern: if sanctuary policies attract populations that consume more in public services than they contribute in tax revenue, mobile high-income households will exit, eroding the tax base and generating a fiscal death spiral.

Empirical work on the fiscal effects of immigration generally finds modest net fiscal impacts at the local level (NRC, 2017). Watson (2014) and Potochnick et al. (2017) document a "chilling effect" in which enforcement deters immigrants — including those legally eligible — from accessing public services, reducing rather than increasing fiscal costs. Alsan and Yang (2024) show that Secure Communities reduced SNAP and SSI participation among Hispanic citizen households in enforcing jurisdictions, confirming that enforcement effects spill over from the targeted unauthorized population to the broader immigrant community.

We contribute to this literature by testing whether enforcement displacement generates fiscal costs in *receiving* jurisdictions rather than in the enforcing jurisdictions themselves. The existing literature focuses on fiscal effects where enforcement occurs; we ask whether there are fiscal consequences where the displaced population arrives.

2.7 The Gap

Despite the rapid growth of the enforcement-labor market literature, no paper has estimated the *cross-jurisdictional* labor market effects of immigration enforcement. The spatial decomposition — distinguishing own-county from neighbor-county effects — is entirely absent from the wages and employment literature, although it has been applied to crime (Kang and Song, 2022). The 287(g)-specific labor market literature is particularly thin: Bohn and Santillano (2017) use a contiguous-county border comparison to estimate 287(g) effects on local employment, finding increased manufacturing employment but reduced administrative services employment. Their design compares enforcing counties to their non-enforcing neighbors, but it does not estimate separate own and neighbor spillover coefficients in a spatial model. No paper has simultaneously estimated own-county and neighbor-county enforcement effects on wages and employment, nor has any paper tested whether the labor market costs of enforcement are borne locally or exported to neighbors. This is the gap we fill.

3 Institutional Background: 287(g) Agreements

Section 287(g) of the Immigration and Nationality Act, added by the Illegal Immigration Reform and Immigrant Responsibility Act of 1996, authorizes the federal government to enter into agreements with state and local law enforcement agencies, delegating limited immigration enforcement authority to local officers. Three models of 287(g) have operated over the program's history. The *Task Force Model* (TFM) authorized local officers to question and detain individuals in the field; it was the most aggressive form of enforcement and was terminated in 2012 under the Obama administration. The *Jail Enforcement Model* (JEM) authorized local officers to screen individuals already in custody, and the *Warrant Service Officer* (WSO) model authorized local officers to serve immigration detainers. JEM and WSO remain the active program models.

Several features of the 287(g) program make it well-suited for studying spatial externalities. First, adoption is *voluntary*: unlike Secure Communities, which was federally mandated and eventually activated in all jurisdictions, 287(g) requires a local law enforcement agency to apply for and negotiate an agreement with ICE. Approximately 120 counties have activated 287(g) at some point during our sample period, concentrated in the Southeast, Mountain West, and parts of the Midwest. This selective adoption generates geographic clusters of enforcing and non-enforcing jurisdictions — precisely the variation needed to estimate spillover effects.

Second, 287(g) adoption is *staggered*: agreements were signed at different times between 2006 and 2024, with waves of adoption under different presidential administrations. A small number of early adopters signed agreements in 2006–2008; a larger wave followed in 2008–2012; the program contracted under the Obama administration (2012–2016); and significant expansion occurred under the Trump administration (2017–2021 and 2025–present). This temporal variation allows us to control for time-invariant county characteristics and time-varying state-level confounders.

Third, 287(g) agreements are *reversible*: approximately 25 counties have had agreements terminated, either voluntarily or through federal policy changes. The Obama administration terminated all TFM agreements circa 2012, and several JEM agreements were terminated during 2020–2021. Six counties are classified as "reversals" — they terminated an agreement and later re-signed. Our treatment variable accounts for these terminations and reversals using spell-based coding, so that $g287\_active$ is 1 only during periods when a county has an active agreement and 0 otherwise.

Fourth, the geographic clustering of 287(g) creates natural treatment and control groups for spatial analysis. When a county's neighbor activates 287(g), the focal county experiences a change in enforcement exposure that is plausibly exogenous to its own characteristics, conditional on county and state-by-year fixed effects. The key identifying assumption is that a county's own unobserved time-varying characteristics do not determine whether its neighbors adopt 287(g), after conditioning on the fixed effects structure.

Barrera et al. (2025) provide evidence relevant to the selection process: they find that the Great Recession increased the likelihood of 287(g) adoption, with effects concentrated in commuting zones with above-median immigrant populations. This suggests that economic conditions and immigrant population shares influence adoption — factors that our county fixed effects and state-by-year fixed effects absorb. The remaining concern is that time-varying county-level shocks might simultaneously drive a county's neighbors to adopt 287(g) and affect the county's own labor market. We address this concern through our fixed effects structure and through robustness checks that control for own-county enforcement status.

Figure 1 displays the geography of 287(g) enforcement and neighbor exposure as of 2020. The geographic clustering of enforcement in the Southeast and Mountain West is immediately apparent, as are the rings of enforcement exposure in non-enforcing counties that neighbor active 287(g) jurisdictions. It is these exposed, non-enforcing counties — shaded in blue — that constitute the primary units of analysis for our spatial externality estimates.

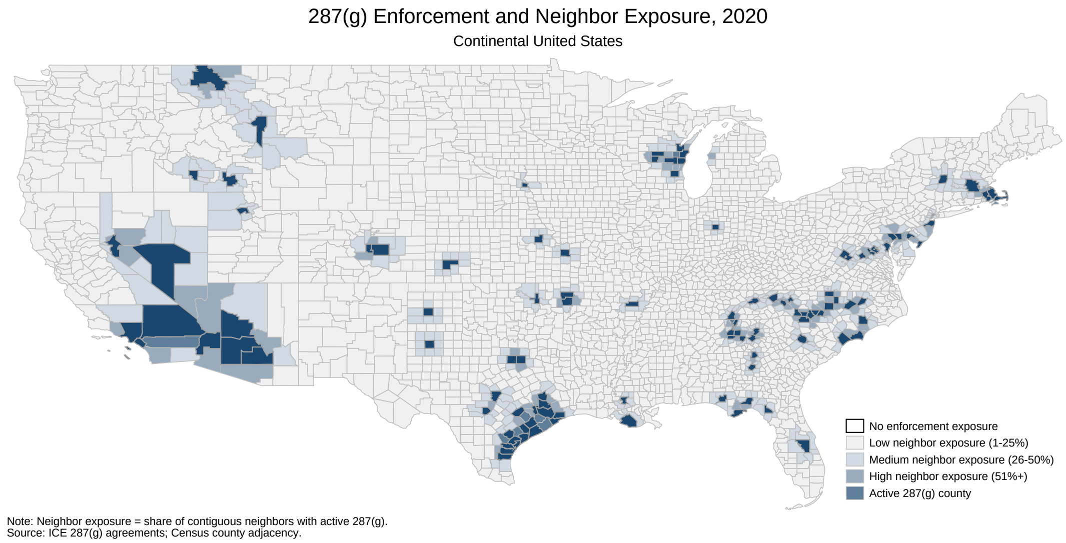

Figure 1: 287(g) Enforcement and Neighbor Exposure, 2020

Notes: Continental United States. Counties are shaded in a sequential navy palette by enforcement exposure. The darkest navy indicates counties with active 287(g) agreements in 2020. Lighter shades indicate non-enforcing counties by share of contiguous neighbors with active 287(g): light (1–25%), medium (26–50%), dark (51%+). White indicates no enforcement exposure. Source: ICE 287(g) agreements; Census county adjacency file.

4 Theory

4.1 Enforcement as Spatial Reallocation

Immigration enforcement does not eliminate immigrant populations from the national labor market; it reallocates them across jurisdictions. When a county activates a 287(g) agreement, it acquires the authority to identify and detain undocumented residents during routine law enforcement encounters. The deterrent effect extends beyond the directly targeted population: documented immigrants, mixed-status families, and even U.S.-born Hispanics reduce their engagement with local institutions, relocate to other jurisdictions, or avoid entering enforcing counties altogether (Watson, 2014; Amuedo-Dorantes et al., 2019; Dee and Murphy, 2018; Alsan and Yang, 2024). The relevant question for economic incidence is not whether enforcement deters immigrants — the evidence is clear that it does — but where the deterred population goes, and what happens to the labor markets and housing markets that receive it.

We distinguish between two spatial channels through which enforcement effects propagate. The *own-county channel* operates within the enforcing jurisdiction: enforcement removes or deters immigrant workers, reducing labor supply and potentially raising wages for remaining workers (the direct "labor scarcity" effect). The *neighbor-county channel* operates in adjacent jurisdictions: displaced workers relocate to the nearest non-enforcing counties, increasing labor supply and potentially depressing wages (the spatial externality).

4.2 Why Expect Spillovers? Why Not?

Whether enforcement in one county generates measurable economic consequences in its contiguous neighbors is theoretically contested. We organize the argument around five mechanisms that predict spillovers and five considerations that push against them. The ex ante sign and magnitude of the neighbor-county effect are therefore an empirical question.

We identify five reasons to expect cross-jurisdictional spillovers. First, Tiebout-style sorting under enforcement risk: the residential location decisions of immigrant households are responsive to local deportation risk (Watson, 2014; Amuedo-Dorantes et al., 2019), and the lowest-cost adjustment is to relocate to the nearest non-enforcing jurisdiction while preserving access to existing labor-market ties. Second, the neighbor-county labor supply shock: an inflow of displaced workers raises effective labor supply in adjacent counties, mechanically depressing wages in sectors with inelastic labor demand. Third, employer relocation and cross-boundary hiring: firms in immigrant-reliant sectors — construction, agriculture, hospitality, administrative services — may shift hiring or operations across the county line, exporting the wage incidence alongside the workforce. This is the direct analog of the domestic pollution-haven effect documented by Kahn and Mansur (2013). Fourth, commuting withdrawal: workers residing in an enforcing county may abandon cross-boundary commutes that pass through enforcement-active jurisdictions, distorting labor supply at the regional scale. Fifth, network and information spillovers: co-ethnic networks transmit enforcement information across short distances faster than long distances, producing discrete spatial discontinuities at county-adjacency boundaries even absent observed mobility.

Five considerations work in the opposite direction. First, 287(g) is legally targeted: its reach is confined to the adopting county's jail bookings and task-force encounters, and its enforcement authority stops at the county line. Second, commuting costs, family ties, schools, and housing tenure anchor residence; relocating even one county over is costly, and the marginal mover may be rare. Third, informal labor markets can absorb shocks locally: cash employment, under-the-table arrangements, and extended-family household structures reallocate labor without cross-boundary flows (Borjas and Cassidy, 2019). Fourth, neighbor counties may lack the industrial complementarity required to absorb displaced workers at similar wages: if the enforcing county and its neighbors have dissimilar sectoral compositions, there is no receiving labor market at the adjacent margin. Fifth, general-equilibrium offsets may attenuate the observable spillover: as rents rise in receiving counties and fall in enforcing ones, relative wages and rents adjust across space to restore indifference (Roback, 1982; Moretti, 2011; Monras, 2020). A related concern is selection. 287(g) adoption is voluntary and geographically clustered (Barrera et al., 2025), so enforcing counties and their neighbors share unobserved trends that, if unaddressed, would attenuate rather than inflate identified spillovers.

The relative strength of these competing mechanisms determines whether a spatial externality from 287(g) is detectable in the data. We argue that the balance tilts toward measurable spillovers in the specific setting of voluntary county-level enforcement, because the policy creates precisely the geographic patchwork — enforcing counties bordered by non-enforcing neighbors — in which the sorting and labor-supply channels can operate. In the universal-rollout setting of Secure Communities, by contrast, the patchwork does not exist and the spillover channels are mechanically shut down. This distinction between voluntary, selective enforcement and universal, mandatory enforcement is the conceptual foundation of our empirical predictions.

4.3 Predictions

The spatial decomposition generates three predictions that differentiate our framework from the within-jurisdiction approach of East et al.

Prediction 1: Own enforcement has no effect on own-county wages. This prediction may seem counterintuitive, given that enforcement reduces the local immigrant labor force. The null follows from two considerations. First, 287(g) counties are a small fraction of the national labor market (120 of 3,100 counties), and the immigrant workers displaced from any single county represent a marginal reduction in local labor supply. Second, the displaced workers do not leave the regional labor market — they relocate to adjacent jurisdictions, often within the same commuting zone. To the extent that local labor markets are integrated across county boundaries, the effective labor supply in the enforcing county may not change, even as its composition shifts. East et al.'s finding that SC reduced wages within enforcing CZs reflects the fact that SC was universal — there were no nearby non-enforcing jurisdictions to absorb displaced workers.

Prediction 2: Neighbor enforcement depresses wages in non-enforcing counties, particularly in immigrant-intensive sectors. When multiple counties in a region adopt 287(g), displaced workers concentrate in the remaining non-enforcing counties, increasing effective labor supply. If the displaced workers are disproportionately low-skilled and employed in sectors with high immigrant labor shares — agriculture, construction, food services, administrative and waste services (Passel and Cohn, 2016) — the wage effects should be concentrated in these sectors. The magnitude depends on the elasticity of labor demand and the volume of displacement.

Prediction 3: Enforcement raises rents in both own and neighboring counties. In the enforcing county, enforcement may reduce the supply of rental housing seekers (as immigrants depart), but it may also reduce the supply of rental housing (as immigrant-owned or immigrant-operated rental properties exit the market). The net effect on rents is ambiguous. In neighboring counties, the effect is clearer: an inflow of displaced population increases housing demand, bidding up rents. We expect positive rent effects that are diffuse across both enforcing and non-enforcing jurisdictions.

4.4 Sanctuary Status and Heterogeneity

Sanctuary jurisdictions — counties that formally limit cooperation with federal immigration enforcement — represent a theoretically important source of heterogeneity. If undocumented populations sort disproportionately into sanctuary jurisdictions (where deportation risk is lower), while documented families sort into non-sanctuary jurisdictions (where institutional engagement is safer), then the composition of the displaced population differs across receiving jurisdictions. We test for this heterogeneity by interacting enforcement exposure with sanctuary status, generating a difference-in-difference-in-differences (DDD) design. In the main analysis, however, the spatial asymmetry between own and neighbor effects operates regardless of sanctuary status.

5 Data

We assemble a county-by-year panel covering 3,141 counties over the period 2010–2023. This section describes each data source and the key variables constructed from it.

5.1 287(g) Enforcement Data

We code the activation date of every 287(g) agreement (Jail Enforcement Model, Warrant Service Officer, and Task Force Model) from ICE administrative records, generating a county-by-year binary indicator $g287\_active$ and a spell-based treatment history that accounts for agreement terminations and reversals. Approximately 120 counties activate 287(g) at some point during the sample period. The spatial enforcement exposure variable w_g287_active — the share of contiguous neighbors with an active agreement — is constructed from the Census Bureau's county adjacency file using queen contiguity weights (two counties are neighbors if they share any boundary segment). We also construct $w\_n\_enforcing$, the count of enforcing neighbors, as an alternative intensity measure.

5.2 Sanctuary Policy Data

County-level sanctuary status is coded from the Immigrant Legal Resource Center (ILRC) database and supplementary legal research. The time-varying indicator identifies approximately 150 counties with active sanctuary policies at some point during the sample period. For the main analysis, we use an ever-sanctuary indicator ($sanctuary\_ever$) that is time-invariant and predetermined for the majority of sanctuary counties.

5.3 Labor Market Data

County-level annual employment, average weekly wages, and establishment counts by NAICS sector are drawn from the Bureau of Labor Statistics' Quarterly Census of Employment and Wages (QCEW). We focus on six sectors selected for their intensity of immigrant labor: Total Private (NAICS 10), Agriculture (11), Construction (23), Manufacturing (31–33), Administrative and Waste Services (56), and Accommodation and Food Services (72) (Passel and Cohn, 2016; Peri and Sparber, 2009). All wage and employment variables are log-transformed; counties with zero sectoral employment are coded as missing. Coverage varies by sector: total private wages are available for 43,654 county-year observations, while agriculture is available for 23,454.

5.4 Housing Data

County-level median gross rent and rental vacancy rates are drawn from the American Community Survey (ACS) 5-year estimates.

5.5 School Enrollment Data

English Language Learner (ELL) enrollment counts are drawn from the National Center for Education Statistics (NCES) Common Core of Data (CCD), available 2009–2021. ELL enrollment provides a direct measure of documented immigrant family presence: children of undocumented parents can enroll in public schools, but ELL identification requires parental engagement with the school system. We construct ln(ELL) and ELL share of total enrollment as outcome variables.

5.6 Fiscal and Social Indicators

Child poverty rates (ages 5–17) and median household income are drawn from the Census Small Area Income and Poverty Estimates (SAIPE). Free and reduced-price lunch (FRPL) counts are from the NCES CCD. SNAP benefit expenditures are from the Bureau of Economic Analysis (BEA). Jail incarceration rates are from the Vera Institute of Justice. Property crime rates are from the FBI Uniform Crime Reports. Uninsured rates are from the Census Small Area Health Insurance Estimates (SAHIE). These variables are used primarily in auxiliary analyses testing for fiscal burden effects.

5.7 Migration Data

Bilateral county-to-county migration flows are drawn from the IRS Statistics of Income migration files (2010–2022), which report the number of tax returns and adjusted gross income associated with address changes. State-level migration by AGI bracket is used for Tiebout sorting tests.

5.8 Controls

All specifications include log population and the county unemployment rate as time-varying controls. County fixed effects absorb all time-invariant county characteristics. State-by-year fixed effects absorb all time-varying state-level confounders.

6 Research Design

6.1 Estimating Equation

Our core estimating equation takes the form:

$$Y_{it} = \beta_1 \, g287_{it} + \beta_2 \, \mathbf{W} \cdot g287_{it} + \mathbf{X}'_{it}\gamma + \alpha_i + \delta_{st} + \varepsilon_{it} \tag{1}$$

where Y_{it} is the outcome of interest in county i in year t; $g287_{it}$ is a binary indicator for whether county i has an active 287(g) agreement (the *own enforcement* variable); $\mathbf{W} \cdot g287_{it}$ is the share of county i's contiguous neighbors with an active 287(g) agreement (the *neighbor enforcement* variable); $\mathbf{X}_{it}$ is a vector of time-varying controls (log population, unemployment rate); α_i are county fixed effects; and δ_{st} are state-by-year fixed effects. Standard errors are clustered at the county level throughout.

The coefficient β_1 captures the own-county effect of enforcement on outcome Y; β_2 captures the neighbor-county spillover. Our theoretical framework predicts $\beta_1 \approx 0$ and $\beta_2 < 0$ for wages (enforcement has no own effect but depresses neighbor wages), and $\beta_2 > 0$ for rents (enforcement raises neighbor rents). The simultaneous inclusion of own and neighbor enforcement is critical: omitting $g287_{it}$ from a specification that includes $\mathbf{W} \cdot g287_{it}$ would confound own enforcement effects (for counties that are themselves enforcers) with the spatial spillover.

To test for heterogeneous effects by sanctuary status, we estimate a triple-difference (DDD) specification:

$$Y_{it} = \beta_1 \mathbf{W} \cdot g287_{it} + \beta_2 \,\text{Sanctuary}_i + \beta_3 \left(\mathbf{W} \cdot g287_{it} \times \text{Sanctuary}_i\right) + \mathbf{X}'_{it}\gamma + \alpha_i + \delta_{st} + \varepsilon_{it} \quad (2)$$

where the DDD coefficient β_3 captures whether the spillover effect differs between sanctuary and non-sanctuary counties.

6.2 Identification

Three features of this design support causal interpretation.

County fixed effects absorb all time-invariant county characteristics — geography, historical settlement patterns, industrial composition, baseline demographics — that might confound the enforcement-outcome relationship. The identifying variation comes from within-county changes in enforcement exposure over time.

State-by-year fixed effects absorb all time-varying state-level confounders, including state immigration legislation, economic cycles, and political dynamics that coincide with enforcement adoption. Identification comes only from differential enforcement exposure across counties within the same state and year.

Neighbor rather than own enforcement. The primary treatment variable is neighbor enforcement exposure, not a county's own enforcement decision. A county's own characteristics do not directly determine whether its neighbors adopt 287(g), conditional on the fixed effects structure. This addresses the endogeneity concern that East et al. (2023) raise regarding 287(g): voluntary adoption reflects local political preferences that may correlate with economic conditions. Our design sidesteps this by using the neighbor's choice rather than the county's own choice.

6.3 Pre-trends

We test the parallel trends assumption using an event study design for ELL enrollment — our primary measure of population reallocation. We identify the first year each county gains an enforcing neighbor and estimate dynamic treatment effects relative to the year before treatment. The pre-treatment coefficients (event time -5 through -1) are individually and jointly insignificant ($p > 0.40$ for all pre-period interactions), confirming that sanctuary and non-sanctuary counties followed parallel ELL trajectories before their neighbors began enforcing. Post-treatment, ELL enrollment diverges sharply: the non-sanctuary effect rises to $+0.14$ at event time 0 ($p < 0.01$) and continues growing, while the sanctuary interaction is large and negative, producing a net sanctuary effect near zero.

7 Results

We present results in four subsections. Section 7.1 reports the main wage results, including the own-versus-neighbor decomposition. Section 7.2 documents housing market effects. Section 7.3 traces the population reallocation mechanism through school enrollment. Section 7.4 tests for fiscal burden effects. Employment and establishment results are reported alongside the wage analysis; Tiebout sorting tests are in the appendix.

7.1 Wage Effects: The Spatial Asymmetry

Table 1 presents the first key result: own 287(g) enforcement has no detectable effect on wages in the enforcing county. We estimate Equation 1 with only own enforcement ($g287\_active$) as the treatment variable, controlling for log population and unemployment rate, with county and state-by-year fixed effects. The own-enforcement effect on total private-sector wages is statistically indistinguishable from zero, and the null holds across all five sectors — construction, agriculture, accommodation and food services, and administrative and waste services. Own enforcement also has no detectable effect on employment in any sector.

Table 2 presents the contrast: neighbor enforcement significantly depresses wages. Using the DDD specification (Equation 2), a shift from zero to full enforcement among a county's neighbors reduces total private-sector wages by approximately 3.1 percent. The sanctuary interaction is statistically insignificant, indicating no differential effect in sanctuary counties: wages fall equally in all counties exposed to neighbor enforcement, regardless of their own sanctuary status.

Table 3 presents the definitive test: own and neighbor enforcement entered simultaneously in Equation 1. For total private-sector wages, the own-enforcement effect remains indistinguishable from zero, and the neighbor-enforcement effect remains a statistically significant 3.1 percent wage decline. The contrast is the central finding of the paper: the own effect is precisely zero, and the neighbor effect is robust to its inclusion. Construction wages show no detectable effect from either own or neighbor enforcement when both are included. Employment effects are not statistically significant for either own or neighbor enforcement in any sector.

Figure 2 displays the spatial asymmetry visually. Across all four sectors, the own-enforcement coefficients (blue circles) cluster tightly around zero, while the neighbor-enforcement coefficients (red diamonds) pull consistently negative. The contrast is starkest for total private-sector wages, where the neighbor effect is precisely estimated and the own effect is indistinguishable from zero. This figure encapsulates the paper's central finding: the labor market costs of 287(g) enforcement are not borne by the enforcing county — they are exported to its neighbors.

The sector-specific results are informative about the channel through which neighbor enforcement operates. In the DDD specification, the food services sector shows the strongest evidence of wage effects: neighbor enforcement raises food service wages by approximately 2.9 percent in non-sanctuary counties, but this positive effect is fully offset, by approximately 4.8 percentage points, in sanctuary counties. The sign pattern in agriculture and administrative services is negative but imprecisely estimated, reflecting the high variance in these sectors

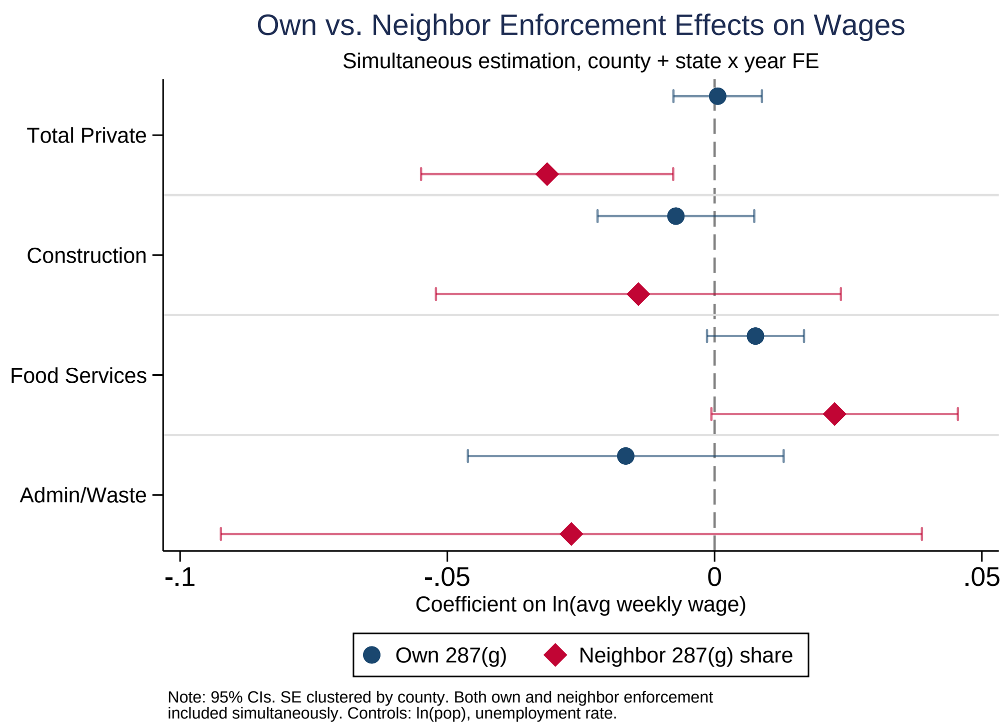

Figure 2: Own vs. Neighbor Enforcement Effects on Wages

Notes: Point estimates and 95% confidence intervals from simultaneous estimation of own 287(g) status and neighbor 287(g) share, with county and state×year fixed effects. Standard errors clustered by county. Controls: ln(population), unemployment rate. Blue circles: own-county 287(g) coefficient. Red diamonds: neighbor-county 287(g) share coefficient.

at the county level.

Figure 3 summarizes the sector-specific pattern in a two-panel coefficient plot. Panel A displays the base effect of neighbor enforcement on wages across all counties; Panel B displays the additional sanctuary interaction. The base effects are uniformly negative or near zero (with food services the exception), while the sanctuary interactions are mixed in sign and generally imprecise — confirming that the wage depression operates broadly rather than being concentrated in sanctuary jurisdictions.

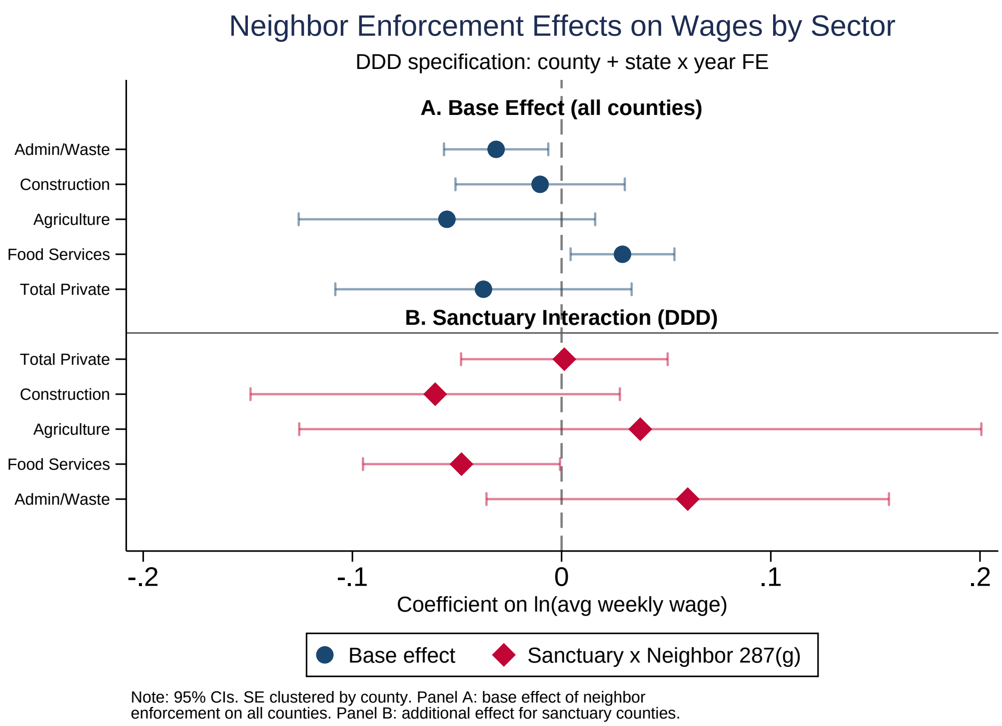

Figure 3: Neighbor Enforcement Effects on Wages by Sector

Notes: Point estimates and 95% confidence intervals from the DDD specification (Equation 2). Panel A: base effect of neighbor enforcement share on ln(average weekly wage) for all counties. Panel B: additional effect for sanctuary counties. County and state×year fixed effects. Standard errors clustered by county.

The employment results (Table 6) show a parallel pattern with less precision. Total private employment and the sanctuary interaction are both marginally signed but statistically indistinguishable from zero at conventional levels. Sector-specific employment effects are uniformly insignificant, reflecting the difficulty of identifying sectoral employment effects at the county level.

7.2 Housing Market Effects

Table 5 reports rent and migration results from the DDD specification. Both the base and sanctuary-interaction effects on ln(rent) are positive but statistically insignificant once state-by-year fixed effects are included. The pattern is directionally consistent with enforcement raising rents, although the effect is absorbed by the stringent fixed effects structure.

The migration variables provide clearer evidence of population reallocation. Gross outflow from counties near enforcers rises by approximately 29 tax returns per year, and gross inflow rises by approximately 32 returns. Net migration rises by roughly 3.5 returns, indicating that counties near enforcers gain population on net. The sanctuary interaction on net migration is approximately -9.0 returns and statistically significant, fully reversing the positive base effect: sanctuary counties near enforcers experience reduced net inflow rather than the net gains observed in non-sanctuary counties. Rental vacancy rates show no detectable change, indicating that the population inflow is absorbed by the existing housing stock without raising vacancy.

7.3 Population Reallocation: The ELL Channel

Table 4 traces the population reallocation mechanism through English Language Learner enrollment. In the DDD specification, a shift from zero to full enforcement among a county's neighbors raises ELL enrollment by approximately 22 percent in non-sanctuary counties. The sanctuary interaction is large, negative, and statistically significant, fully offsetting the base effect: ELL enrollment rises outside sanctuary jurisdictions; it does not rise within them.

This pattern is confirmed in split-sample analysis. Among non-sanctuary counties ($N =$ 34,776), neighbor enforcement raises ELL enrollment by approximately 20 percent. Among sanctuary counties ($N =$ 1,635), the estimate is statistically indistinguishable from zero. Own enforcement has no effect on own-county ELL enrollment, ruling out a mechanical relationship. The documented families displaced by 287(g) are enrolling their children in *non-sanctuary* school districts.

The event study analysis (Figure 4) provides the strongest evidence for a causal interpretation. We identify the first year each county gains an enforcing neighbor and estimate dynamic treatment effects relative to $t = -1$, separately for non-sanctuary and sanctuary counties. The pre-treatment coefficients ($t = -5$ through $t = -1$) are individually and jointly insignificant for both groups, confirming parallel trends. Post-treatment, the series diverge sharply: non-sanctuary counties (blue circles) show ELL enrollment rising immediately at $t = 0$ and continuing to grow through $t + 5$, while sanctuary counties (red diamonds) remain flat or decline. The sharp break at the treatment date and the absence of pre-treatment divergence are consistent with a causal effect of neighbor enforcement on ELL enrollment in non-sanctuary jurisdictions.

7.4 Fiscal Burden Tests

We test whether the population reallocation documented above generates measurable fiscal costs in receiving jurisdictions. Across eight fiscal indicators — child poverty rate, pupil-teacher ratio, all-ages poverty rate, FRPL share, jail incarceration rate, jail population, jail admissions, and SNAP benefits per capita — not a single DDD estimate is statistically significant at conventional levels (Figure 5). The fiscal null is comprehensive.

Two health indicators show significant DDD effects, but in the direction *opposite* to the fiscal burden prediction: the uninsured rate falls by approximately 1.9 percentage points and teen birth rates fall by approximately 4 per 1,000 in sanctuary counties near enforcers. These findings are inconsistent with a fiscal burden story and may reflect compositional effects — the populations arriving in sanctuary jurisdictions may be selected on health or risk aversion.

Property crime presents a similar pattern: the DDD estimate for burglary is large, negative, and statistically significant, suggesting that property crime falls in sanctuary counties exposed to neighbor enforcement. We flag the magnitude of this estimate as potentially re-

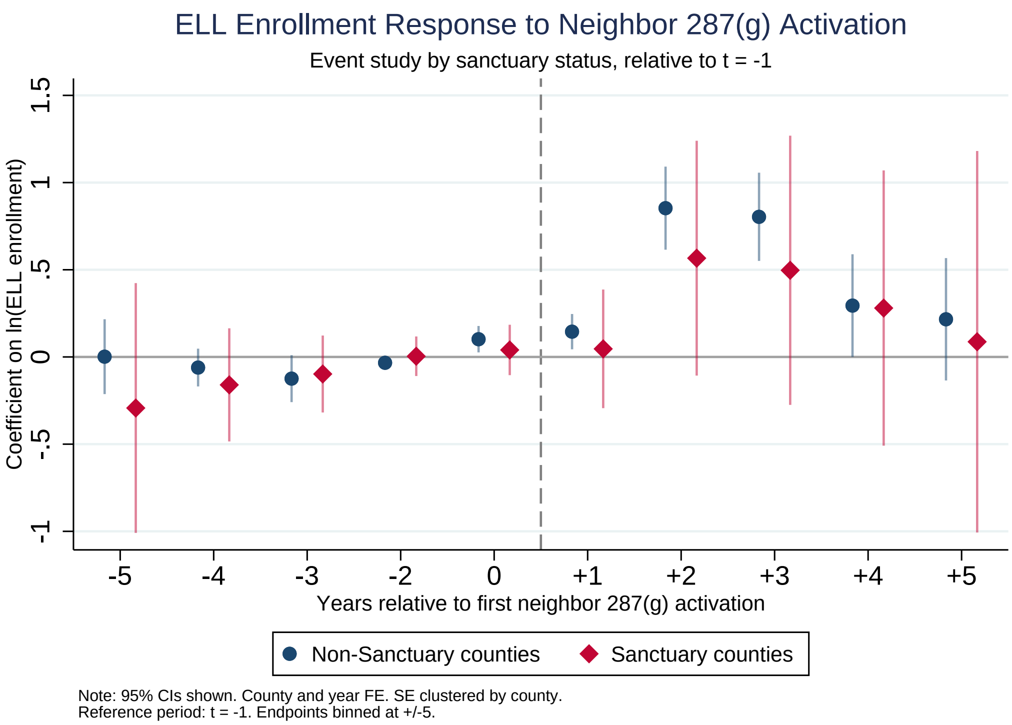

Figure 4: ELL Enrollment Response to Neighbor 287(g) Activation

Notes: Event study estimates of ln(ELL enrollment) relative to $t = -1$ (the year before a county first gains an enforcing neighbor). Sample restricted to counties that eventually gain an enforcing neighbor (N = 7,051; 510 counties, of which 48 are sanctuary). Blue circles: non-sanctuary counties. Red diamonds: sanctuary counties. 95% confidence intervals shown. County and year fixed effects. Standard errors clustered by county. Endpoints binned at ± 5.

flecting measurement issues in the UCR data and do not emphasize it in the main analysis.

Figure 5 summarizes the fiscal battery as a forest plot of t-statistics. Eight of the nine DDD interaction coefficients fall well within the ± 1.96 significance bounds (hollow circles). The sole exception is the burglary rate (red filled circle), which is significant but in the direction *opposite* to the fiscal burden prediction — crime *falls* in sanctuary counties exposed to neighbor enforcement. The visual uniformity of the null across poverty, school finance, incarceration, SNAP, and crime indicators is more persuasive than any individual coefficient: enforcement displacement simply does not register in the administrative systems that produce fiscal data.

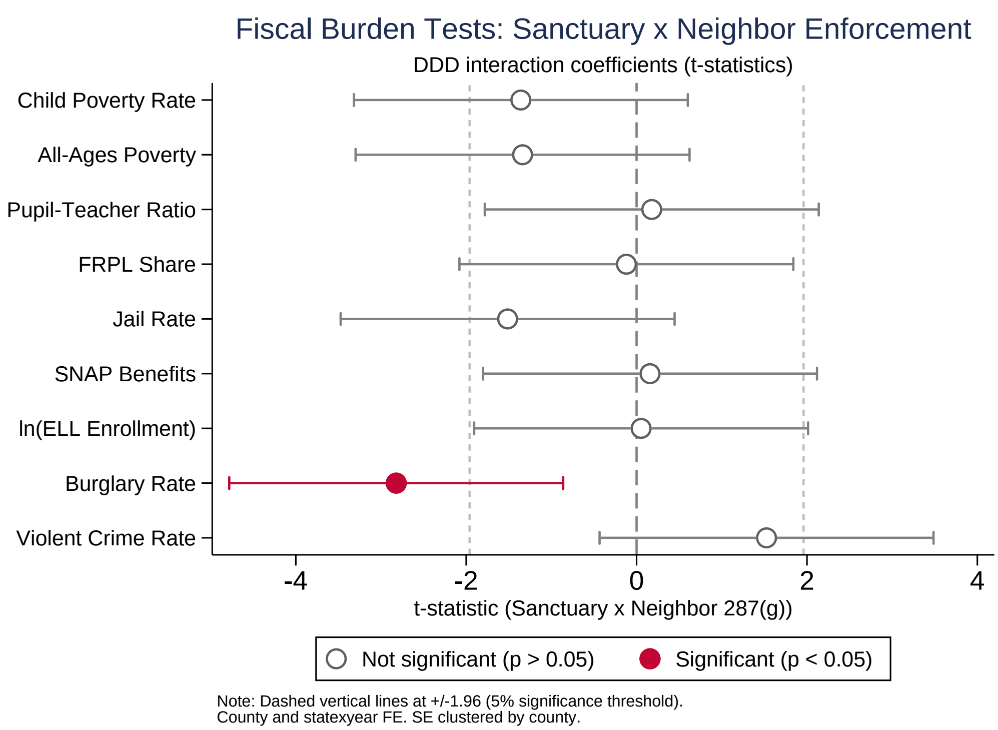

Figure 5: Fiscal Burden Tests: DDD Interaction Coefficients

Notes: Each point represents the t-statistic on the sanctuary $\times$ neighbor enforcement interaction from the DDD specification (Equation 2). Hollow circles: not significant ($p > 0.05$). Filled red circles: significant ($p < 0.05$). Dashed vertical lines at ± 1.96. County and state$\times$year fixed effects. Standard errors clustered by county.

The fiscal null does not mean that enforcement displacement has no consequences — it means that the consequences are not captured by the administrative systems that produce fiscal indicators. The displaced population that arrives in nearby counties is disproportionately undocumented, ineligible for most means-tested programs, and reluctant to engage with government institutions. Their arrival affects labor markets and housing markets — where their presence is registered through wages and rents — but not the fiscal indicators that drive the political debate over immigration enforcement.

8 Discussion and Conclusion

Who bears the economic costs of local immigration enforcement? The standard approach in the literature estimates the effect of enforcement within the jurisdiction that adopts it, implicitly assuming that the costs of enforcement are borne locally. We show that this assumption is wrong for 287(g) agreements: the labor market costs of enforcement are spatially exported to neighboring jurisdictions.

The core finding is a spatial asymmetry: own 287(g) enforcement has no detectable effect on wages or employment in the enforcing county, but neighbor enforcement depresses total private-sector wages by approximately 3.1 percent. When own and neighbor enforcement are included simultaneously, the own-county effect remains indistinguishable from zero and the neighbor-county effect remains a statistically significant 3.1 percent decline. This asymmetry is consistent with a model in which enforcement displaces low-skilled workers into adjacent labor markets, where they increase effective labor supply and depress wages. The enforcing county avoids the wage cost because the displaced workers leave, but they do not leave the regional labor market — they move next door.

This finding reframes the policy debate in an important way. The political logic of 287(g) adoption is straightforward: local officials can signal toughness on immigration and claim credit for enforcement, while the economic costs are borne by their neighbors. The externality is real: a county's decision to enforce creates a negative labor market spillover for adjacent jurisdictions that had no voice in the decision. This is a classic spatial externality of the kind that motivates higher-level government coordination (Oates, 1999). If the costs of enforcement are exported rather than borne locally, then the counties that choose to enforce are not internalizing the full social cost of their policy choice.

Our results also illuminate why the crime effects of enforcement collapse under stringent fixed effects while the labor market effects survive. Crime — measured at annual frequency with state-by-year fixed effects — shows no relationship to enforcement in either own or neighbor counties. Labor markets, by contrast, respond through a fundamentally different mechanism: the physical reallocation of workers across jurisdictional boundaries. State-by-year fixed effects absorb state-level trends in crime that happened to correlate with enforcement timing (early-adopting southern states had different crime trajectories), but they do not absorb the within-state, across-county reallocation of labor that produces our wage effects.

Several limitations merit discussion. First, our wage data come from the QCEW, which covers formal-sector employment. If displaced workers are disproportionately employed in the informal sector, our estimates understate the true labor supply shock and may miss wage effects in informal markets. Second, the spatial weight matrix uses simple queen contiguity, which treats all neighbors equally regardless of population, economic integration, or transportation links. A commuting-zone-based weight matrix might capture labor market spillovers more accurately, although it would sacrifice the fine geographic resolution that county-level analysis provides. Third, our panel covers 2010–2023, a period that includes the Great Recession recovery, the COVID-19 pandemic, and substantial changes in federal immigration policy. Our state-by-year fixed effects absorb these macro shocks, but substate heterogeneity in their effects could confound our estimates.

We close by noting the broader implications for immigration policy design. The federal government delegates enforcement authority to local jurisdictions through 287(g), but it does not compensate neighboring jurisdictions for the economic costs that enforcement generates. The spatial externality we document suggests that efficient policy design would either (1) internalize the externality by compensating neighboring jurisdictions, (2) coordinate enforcement at a level (e.g., the commuting zone) that encompasses both the enforcing jurisdiction and its labor market neighbors, or (3) restrict the delegation of enforcement authority to prevent local jurisdictions from exporting costs to their neighbors. The current design, in which enforcement costs are invisible to the jurisdictions that choose to enforce, creates incentives for excessive local enforcement — a spatial race to the bottom in which each county's enforcement decision imposes uncompensated costs on its neighbors.

Table 1: Own 287(g) Enforcement and Own-County Wages

	(1) Total Priv.	(2) Construction	(3) Agriculture	(4) Food Svc.	(5) Admin.
$g287\_active$	-0.001	-0.008	0.009	0.009	-0.018
	(0.004)	(0.008)	(0.015)	(0.005)	(0.015)
Observations	43,654	39,360	23,454	33,995	31,572
R^2 (within)	0.007	0.003	0.001	0.007	0.000

County and state×year fixed effects. Standard errors clustered by county in parentheses. Dependent variable: ln(average weekly wage). Controls: ln(population), unemployment rate.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Neighbor Enforcement and Wages: DDD with Sanctuary Status

	(1) Total Priv.	(2) Construction	(3) Agriculture	(4) Food Svc.	(5) Admin.
$\mathbf{W} \cdot g287$	-0.031^{**}	-0.010	-0.055	0.029^{**}	-0.037
	(0.013)	(0.021)	(0.036)	(0.013)	(0.036)
Sanctuary $\times$ $\mathbf{W} \cdot g287$	0.001	-0.060	0.038	-0.048^{**}	0.060
	(0.025)	(0.045)	(0.083)	(0.024)	(0.049)
Observations	43,654	39,360	23,454	33,995	31,572

County and state×year fixed effects. Standard errors clustered by county in parentheses. Dependent variable: ln(average weekly wage). Controls: ln(population), unemployment rate. $\mathbf{W} \cdot g287$ is the share of contiguous neighbors with active 287(g) agreements.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Own vs. Neighbor Enforcement: Simultaneous Estimation

	(1) Wages: Total	(2) Wages: Constr.	(3) Employment: Total	(4) Employment: Constr.
$g287\_active$ (own)	0.001	−0.007	0.007	0.011
	(0.004)	(0.007)	(0.012)	(0.049)
$\mathbf{W} \cdot g287$ (neighbor)	−0.031***	−0.014	0.064	0.046
	(0.012)	(0.019)	(0.044)	(0.134)
Observations	43,654	39,360	43,827	43,685
R^2 (within)	0.007	0.003	0.007	0.002

County and state×year fixed effects. Standard errors clustered by county in parentheses. Columns (1)–(2): ln(average weekly wage). Columns (3)–(4): ln(employment). Both own enforcement ($g287\_active$) and neighbor enforcement ($\mathbf{W} \cdot g287$) are included simultaneously.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: ELL Enrollment Response to Neighbor Enforcement

	(1) ln(ELL)	(2) ELL Share	(3) ln(Enrollment)	(4) DDD: Count
$\mathbf{W} \cdot g287$	0.217***	0.007*	0.031*	
	(0.074)	(0.004)	(0.017)	
Sanctuary × $\mathbf{W} \cdot g287$	−0.268**	0.008	−0.029	
	(0.126)	(0.011)	(0.033)	
$w\_n\_enforcing$				0.037***
				(0.014)
Sanctuary × $w\_n\_enforcing$				−0.060**
				(0.026)
Observations	36,534	36,509	36,510	36,534

County and state×year fixed effects. Standard errors clustered by county in parentheses. Controls: ln(population), unemployment rate. Column (4) uses the count of enforcing neighbors rather than the share.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Housing Market and Migration Effects

	(1) ln(Rent)	(2) Outflow	(3) Inflow	(4) Net Migr.	(5) Vacancy
$\mathbf{W} \cdot g287$	0.007	28.61**	32.08**	3.48***	0.62
	(0.013)	(14.39)	(14.62)	(1.16)	(1.22)
Sanct. $\times$ $\mathbf{W} \cdot g287$	0.042	−8.19	−17.09	−9.00***	0.34
	(0.032)	(19.44)	(20.57)	(2.81)	(2.22)
Observations	43,778	40,633	40,624	40,613	43,827

County and state×year fixed effects. Standard errors clustered by county in parentheses. Controls: ln(population), unemployment rate. Outflow, Inflow, and Net Migration are measured in IRS tax returns per year.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Neighbor Enforcement and Employment: DDD with Sanctuary Status

	(1) Total Priv.	(2) Construction	(3) Food Svc.	(4) Agriculture	(5) Admin.
$\mathbf{W} \cdot g287$	0.079*	0.210	−0.065	0.227	−0.028
	(0.047)	(0.172)	(0.171)	(0.260)	(0.244)
Sanct. $\times$ $\mathbf{W} \cdot g287$	−0.152*	−0.126	0.014	−0.310	−0.277
	(0.091)	(0.272)	(0.181)	(0.679)	(0.327)
Observations	43,827	43,685	43,647	43,011	43,020

County and state×year fixed effects. Standard errors clustered by county in parentheses. Dependent variable: ln(employment). Controls: ln(population), unemployment rate.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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